The Question of Logic[1]

Saul A. Kripke
The Saul Kripke Center and the Graduate Center of the City University of New York, United States
kripkecenter@gc.cuny.edu

Under the influence of Quine's famous manifesto, many philosophers have thought that logical theories are scientific theories that can be 'adopted' and tested as scientific theories. Here we argue that this idea is untenable. We discuss it with special reference to Putnam's proposal to 'adopt' a particular non-classical logic to solve the foundational problems of quantum mechanics in his famous paper 'Is Logic Empirical?' (1968), which we argue was not really coherent.

The title might seem to carry the presupposition that logic is unique, and so I think people thought until the modern era – that is, the twentieth century. In the past, when people talked about logic, it was rather rare to use the phrase in the plural. Now, however, this usage is gone. We have a great number of different (or 'alternative', as they like to call them) formal systems, even at the propositional level. Perhaps we can choose to 'adopt' one and later reject it in favour of another. (The

[1] The present paper is based on a lecture delivered at Pittsburgh University in 1974, though in certain places we have followed a seminar given at Princeton University in the same year, and seminars given at the CUNY Graduate Center (co-taught with Romina Birman) in 2014, 2015, and 2020. As usual, we have preserved the conversational tone of the original lecture. Even though the focus of the accompanying papers by Birman, Devitt and Roberts, and Boghossian and Wright is what Padro (now Birman) (2015) has dubbed 'the adoption problem', based on my interpretation of Carroll (1895), we thought it would be appropriate to introduce it to the readers as it was originally presented, that is, in the course of my critical discussion of Putnam (1968). In this broader context, my reading of Carroll is one among the arguments I give against Putnam – though the one I perhaps then (and certainly now) considered the most important.

Enthusiasm for Putnam's proposal for the revision of classical logic in favour of quantum logic as a way of solving the problems of interpreting quantum mechanics has all but disappeared. Putnam himself eventually abandoned this view (see Putnam 1995 and Maudlin 2005). Nevertheless, I hope that my more specific arguments against his views will remain of interest and serve to illustrate some of the many challenges involved in the 'revision' of logic (at least when this is understood in a certain way). In contrast, some of the central ideas of the Quinean doctrine discussed here appear to be experiencing a strong revival, now rebranded as 'anti-exceptionalism' about logic (see Hjortland 2017 and Williamson 2013 and 2017). There are however important differences, both *among* contemporary 'anti-exceptionalists', and between their views and Quine's doctrine.

Although I am very critical of Putnam's earlier proposal in this talk, I wish to emphasize my admiration for him as a philosopher, a logician, and a human being. We came to be in agreement on many important issues, as readers surely know.

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phrase 'to adopt *x* logic' – where the *x* is replaced by your favourite formal system – is in fact ordinarily used as if it had a received philosophical meaning.)[2] Even those who may be against this trend use the term 'logic' as a common noun, rather than as a singular term. There are, after all, lots of logics. The intuitionists, for example (a case I unfortunately won't be able to discuss in detail), are supposed to have chosen to 'adopt' another logic than the received one, and to base a whole different system of mathematics upon it. Many people have heard that this is so, even if they do not – or probably *because* they do not – comprehend the details.

This trend fits nicely with the following popular doctrine in the epistemology of logic: there is no sharp cleavage between logic and other domains of scientific inquiry. They are all of a piece. Logic is just one of the tools we use in organizing our experience. So far, perhaps, classical logic, which is the logic we have originally 'adopted', has well stood the test of experience. No doubt it should not be abandoned for trivial reasons. Maybe it has been well-confirmed, since by using classical logic we have been able to make a large number of predictions, which are well justified. We *could*, however, in principle, and in response to recalcitrant experience, choose to revise our logic rather than to revise something else, like physics or geometry. And perhaps someday experience will lead us to do so.

This is, roughly stated, the Quinean doctrine. Quine himself perhaps held this in theory. In practice, I don't think he thought for a moment that anything but classical logic is of any interest for any purpose whatsoever.[3] But I wish here to discuss an extreme example of this doctrine in a paper promoting the revision of classical logic in favour of 'quantum logic' for empirical reasons, Hilary Putnam's 'Is Logic Empirical?'. At the time of publication, it provoked considerable discussion. Though it is now considered a classic, hardly anyone would count its proposal

[2] 'But the adoption of intuitionistic logic as opposed to "classical" logic amounts to systematically forswearing certain classically valid inferences' (Putnam 1962, p. 51). The phrase continues to be widely used.

[3] I have never seen any signs in Quine's writing that he does: for empirical reasons or any other kind of reasons, if you've got traditional first-order logic, you've got it all. In fact, he gives a brief run over various proposals other than classical quantification theory and is hostile to all of them (Quine 1970, Ch. 6. See also footnote 20 below). For some, as for example the Birkhoff-von Neumann quantum logic, he doesn't even give a technically correct account of what the proposal is supposed to be. (Its main point is to reject the distributive law, not the law of excluded middle, as Quine says.) The finding he ascribes to 'Craig, Henkin, and others' (Quine 1970, p. 91) is due to Ehrenfeucht. (Henkin invented the quantifier in question and proposed the problem; Craig had nothing to do with it.)

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as a way of solving the interpretation problems in quantum mechanics. I have the impression, however, that it is taken sometimes to (at least) have shown something important regarding the revision of logic for empirical reasons (for example, Field 2008, pp. 16-7). Its basic thesis is as follows. Let's assume that A and B are two quantities associated with a physical system, each one with two possible values, 1 and 2:

(1) $A = 1$ or $A = 2$;
(2) $B = 1$ or $B = 2$.

These are two non-commuting quantum mechanical magnitudes for which the Heisenberg uncertainty principle holds. We can think of them as position and momentum. (Though these are not normally given as distinct real numbers, in Putnam's presentation they are.[4] Don't worry if you don't know what all this means!)

The famous principle of complementarity in quantum mechanics says that though one can be sure in advance that a measurement of A will give the value 1 or 2, and that a measurement of B will give the value 1 or 2, you can't measure both. If we measure A and find out that $A = 1$, then determining B's value will be impossible; we will be forever precluded by the experimental set-up from measuring B. A common way of thinking about this is to say that one cannot defend a completely realistic interpretation of these quantities, and thus deny that they have simultaneous values. It is the measurement itself, on this interpretation,[5] that pulls a value into existence: $A = 1$, say, was not determined in advance of our measurement, and B is not measured and never did have value 1 or 2.

Putnam, however, wishes to be a realist. He wants to hold that everything in quantum mechanics is either true or false independently of any measurement; a definite value always existed in advance. But that is not all. He also wants to assert that:

[4] Putnam actually considers two distinct real numbers, r and r', and asserts the analogues of (1) – (6) for them. He imagines position and momentum as given by distinct reals, assuming a one-dimensional space (and acknowledges this as an oversimplification (Putnam 1968, p. 180)). I don't think I have changed the situation by restricting the values to 1 and 2. That could have been the case.

[5] Remember the original date of this lecture. Everett's interpretation of quantum mechanics, for example, was relatively uninfluential then. So was the 'many worlds' idea people derived from Everett. The de Broglie-Bohm interpretation was simply ignored by most of the physics community. The orthodox interpretation was the 'Copenhagen interpretation', as vaguely stated above. There is also other work on measurement in quantum mechanics, unknown then (see Maudlin 2005, p. 164 and Putnam 2005).

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(3) It is not the case that $A = 1$ and $B = 1$;
(4) It is not the case that $A = 2$ and $B = 1$;
(5) It is not the case that $A = 2$ and $B = 2$;
(6) It is not the case that $A = 1$ and $B = 2$.

This would seem to be difficult! Nevertheless, he wants to maintain all of these. To be free to hold (1) – (6) simultaneously, he says, we should 'adopt' a non-standard logic called 'quantum logic', which he takes over from Birkhoff and von Neumann (1936), in which the distributive law[6] – essentially proof by cases (where side premises are allowed) – fails.[7] $(A = 1 \ \vee \ A = 2)$ and $(B = 1 \ \vee \ B = 2)$ do not imply the disjunction of (3) – (6) without the negation sign. That is, Putnam rejects:

$$(A = 1 \wedge B = 1) \vee (A = 1 \wedge B = 2) \vee (A = 2 \wedge B = 1) \vee (A = 2 \wedge B = 2)$$

Let's call the statements (1) – (6) *Putnam's hypothesis*. Suppose $A = 1$. We can then forget about the cases where $A = 2$. But we can still take $A = 1$ and $B = 1 \vee B = 2$. It *really* is one or the other of them, yet it's false that $(A = 1 \ \wedge \ B = 1)$, *and* it's false that $(A = 1 \ \wedge \ B = 2)$. *Hmm* …

I would like to start out with a brief declaration on this issue, leaving all the motivations for quantum mechanics aside for the moment. Suppose I wanted to refute Putnam's hypothesis. How would I do it? You see, I should like, if possible, to have refuted it as follows: since either $A = 1$ or $A = 2$, there are two cases. If $A = 1$, then either $B = 1$ or $B = 2$. It can't be that $B = 1$, because of (3). The other possibility is that $B = 2$, but that is impossible by (6). Since either $B = 1$ or $B = 2$, the only thing that can be wrong is our hypothesis that $A = 1$. Can $A = 2$? We seem to get into the same trouble, because if $B = 1$, that would contradict (4), and if $B = 2$ that would contradict (5). Therefore, $A = 2$ is also ruled out. Since all the cases are exhausted, Putnam's hypothesis is refuted.

I think this is the most conclusive proof that could be given. What could be a more conclusive proof? But anyone who is attracted to Putnam's view is likely to say: 'Look, this begs the question. You are assuming the distributive law, and the whole issue is whether it holds or it can be given up because of empirical evidence'. My reply would

[6] That is, $P \wedge (Q \vee R) \equiv (P \wedge Q) \vee (P \wedge R)$. So, stating things without using a biconditional, in Putnam's proposal $P \wedge (Q \vee R)$ does not entail $(P \wedge Q) \vee (P \wedge R)$.

[7] It is unclear to me what Birkhoff and von Neumann were really proposing. It doesn't seem to be based on the idea of a realistic interpretation of quantum mechanics (nor does von Neumann 1932). They may simply be proposing new connectives based on the formalism of quantum mechanics, somewhat analogous to the classical ones, but not to replace them. *I have no objection to such an idea.* It would not, however, solve any problems about the interpretation of quantum mechanics, since it would presuppose its intelligibility.

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be: 'I was not aware of *assuming* anything called "the distributive law". I deduced a contradiction from these premises *alone*, not from these premises *plus* something else called "the distributive law". On the contrary, *you* are begging the question against *me*. You are saying that there is a missing premise that is required, that unless I assume what you call "the distributive law", my reasoning doesn't follow. I don't see, however, that any extra premise is required. If it were, then of course the argument would be a fallacy or at least a suppressed enthymeme. But I was not using any extra premises, and I haven't begged any question because I have not asserted any premise that Putnam himself doesn't accept. So, if you say that *I* have begged the question, *you yourself* are begging the question against *me*'.[8]

'Ah…' – Putnam's supporter may now say – 'you were assuming an extra rule of inference!' And I'm inclined to reply: 'No. I was not *assuming* any rule of inference. That is just another way of saying that there is an extra and hidden premise, which now you call a "rule of inference". But it doesn't have to be *assumed*. That, once again, is to say that this argument is fallacious without some extra and further assumption. And this is precisely what I deny'.

'In saying that this argument is conclusive by itself' – Putnam's supporter may insist – 'your whole point is that the rules of logic are *a priori*, not that they are tested by experience or known for empirical reasons'. And this too doesn't seem to me to be right either. It is just another way of saying that some *extra* premise – the laws of logic are *a priori* – is needed. Certainly, no such extra and super-philosophical premise is required any more than to compute 3 times 7 we need some premise about whether arithmetic is *a priori*.[9]

I am well aware that anyone attracted to Putnam's views is unlikely to feel very satisfied with this little dialogue. So let me leave it here, except to say this: I don't want to represent myself as giving a better refutation than this one, which – as Putnam would put it – uses the distributive law, or – as I might more neutrally say – conforms to a pattern of inference we identify as disjunction elimination allowing side premises. I couldn't have argued in a more basic way, and I certainly can't give a better refutation of Putnam or anyone else. So, other than saying, 'Look, your views are contradictory, and if you are willing to say "So what?" to

[8] This already should remind the reader of Carroll (1895), where there is an analogous issue about *Modus Ponens*.

[9] I'm not sure whether I was thinking of Moore's proof of an external world here, but my strategy in this argument is in a way similar (though I myself am inclined to think that the two cases are different).

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that, then there is no more that I can say', it seems to me that all one can do is try, as Wittgenstein suggested, a form of therapy. I don't think the philosophy-as-therapy view applies generally, but, in this case, I hope that it can perhaps help get someone out of the mood of finding a certain sweeping philosophical thesis attractive. Still, it won't be a better refutation than the one just given.

Now, how is Putnam planning to get us to accept all the statements (1) – (6) simultaneously? He doesn't give any intuitive argument showing that the reasoning leading to a contradiction is invalid. Instead, he begins with a softening up procedure. It seems to us to be perfectly self-evident that two straight lines both perpendicular to a common line cannot approach each other. General relativity, however, tells us that this is false of the geometry of physical space (actually, space-time), which is non-Euclidean.[10] The intuitive feeling of contradiction in supposing that such straight lines may intersect is no less and no greater than the feeling we get if someone claims that she has a sheet of paper which is scarlet all over and green all over, or that some legally married men at the present time are, nonetheless, still bachelors (Putnam 1968, pp. 174-5).[11]

What Putnam wishes to do with these examples is to subtly (or *not-so-subtly*) undermine intuitive reasoning. Just as in the case of non-Euclidean geometry we throw intuition to the wind and adopt an axiomatic system as supposedly describing the real physical world – in which some situation exists where traditionally parallel lines can somehow sneak in and, if not meet, approach each other, getting closer and closer – so in every other domain we cannot rely on intuition. Once one has a rival system of axioms, the mere fact that an old system of axioms struck us as the only intuitively acceptable one should be given little weight. Once alternative geometries are under consideration, we abandon any near intuitive preference for Euclidean geometry. Once alternative logics are

[10] This statement appears to need qualification based on the way general relativity has developed later, with a strong non-zero cosmological constant.

[11] Actually, I wonder whether we do have 'intuitions' about the geometry of space-time, or that most people have heard of such geometry at all. In Minkowski space-time the geometry is not really Euclidean, but what people have called 'pseudo-Euclidean'. In general relativity the space-time structure is non-Euclidean, or even non-pseudo-Euclidean. It is hard to tell what relativistic theories have to say about 'space' itself. I suppose that if we do try to impose a spatial coordinate, the geometry will be non-Euclidean. See Maudlin (2005), footnote 2, and footnote 9 above. (But maybe the Gödel models of general relativity do not allow any such spatial 'slices'. In any case, my impression is that most people don't think that these models are true.)

Regarding colour I have concluded in my 'No Fool's Red? Reflections on the Primary and Secondary Quality Distinction' that, though it seems *a priori* that nothing can *look* red and green all over, I am not so sure that it is *a priori* that nothing can *be* red and green all over.

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under consideration, we abandon any near intuitive preference for a particular system of logic. And so, if one had a theory of marriage which implied that some – no doubt, only a *few* – of the married men are still bachelors, which satisfied some valuable criteria of internal coherence and predictive power – and it probably would have considerable predictive power – then we might consider it as an alternative to the present system and throw intuition to the wind here too.

Putnam seems to think that no intuitive argument explaining why (1) – (6) don't lead to a contradiction is needed. We, so to speak, stamp our foot and say that, intuition to the contrary notwithstanding, this comes from our finite experience or our warped minds. We just have to 'adopt' a formal system in which the distributive law fails – that is, Birkhoff's and von Neumann's 'quantum logic' – and we won't find all these statements contradictory.[12] The physical evidence indicates that such a system *is* the correct one; otherwise, the interpretation of quantum mechanics becomes problematic.

Parodying Quine's 'semantic ascent', Putnam's strategy here might be called 'methodological ascent'. It consists in raising a methodological issue to neutralize your opponents' arguments and, by doing so, move the discussion to the methodological level. So, your opponents' arguments, usually taken to be correct and even self-evident, are not engaged with directly. They are rejected because of the special methodological considerations being brought up.

This seems quite similar to a move often made in fairly recent philosophy of religion. (In fact, my own interest in this subject originally stems from this area.)[13] Sometimes one finds the following analogous theses. 1) *Any* investigation, scientific, religious, or whatever, must begin with some axioms or unproved assumptions. There is nothing particularly special about religion in this respect. 2) Our creaturely logic doesn't apply to God or the religious domain, which includes paradoxes, statements which human beings, because of their corrupt and creaturely minds, are unable to comprehend. Faith should surmount reason in this domain. (These two ideas are independent; many people might embrace 1) but not 2).)

[12] 'The only laws of classical logic that are given up in quantum logic are distributive laws … and every single anomaly vanishes once we give these up' (Putnam 1968, p. 184).

[13] When I was at a religious summer camp in my early teens, I heard the first doctrine (not the second) advocated by the head of the camp and others. I argued that this cannot apply to logic. If it were based on assumptions, one would never be able to deduce anything from them. Whether or not I was persuasive (probably not), I regard this as one of my earliest philosophical thoughts. And, as we shall see, I still hold it.

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In the old debates in the philosophy of religion a theologian would consider himself refuted if some argument showed that his views were demonstrably false or even improbable (although the phrase '*credo quia absurdum*', apparently misattributed to Tertullian, has been widely discussed). But if one adopts the device of methodological ascent, one can say, 'These criticisms are valid if you are using *reason*, but the whole issue is whether reason is appropriate here'.[14] Similarly, suppose someone is giving a paper and in the discussion section we uncover a contradiction. Ordinarily this would go over fine; it would be thought to be a conclusive refutation of the paper in question. But not, of course, if the speaker had cleverly prepared the remark that she believed that logic can be changed because of some special considerations, and that she is using a non-standard logic. Then she has thought of the objection in advance and is well prepared for it: 'Oh, you're just supposing standard logic. I'm using a non-standard one'.

Aside from the difference in terminology – 'faith versus reason' and 'non-standard logic versus standard logic' – the situation seems to me similar in the two domains. One is supposed to say that strange things are true of God, the other that strange things may be true of electrons. The greater popularity of the electron in analytic circles may be a point of difference. Another is that certain religious writers are content to say that our 'reason' or 'creaturely logic' does not apply to God and leave it at that, whereas here we feel on much more solid ground because we are given a rigorous alternative formal system in which we are supposed to think about electrons (or so we are told). Then one isn't thinking, 'How are we going to be thinking of electrons if not in terms of conventional logic?' One can, however, easily interpret the religious writers as saying that what we would normally consider valid inferences don't apply to God. And if they used a different terminology, they might well say that some non-standard logic applies to God.[15] So both considerations create a strong feeling of difference, but to me the cases are similar.

[14] What can one do? I never knew what to do when someone remarked, 'That's just reason', in a religious discussion. That's where argument seems to cease. If someone tries to find a fallacy in your argument, that is one thing. But if she just says, '*Hmm*, your argument is perfectly correct, but the issue is whether perfectly correct arguments should be accepted', then one does not know what to do.

[15] There has been a fairly vigorous analytic philosophy of religion (usually Christian, but often the issue is simply theism, thought to be defined in some traditional way). I have heard at least one such philosopher advocating the first doctrine ('fideism', or everything has unproved assumptions), but not the second.

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Now, what has happened in the case of logic? First, as I said, the phrase 'alternative logics' – for example, 'alternative propositional calculi' – has gained currency because of the existence of different formal systems even at the elementary level of just the connectives $\wedge$, $\vee$, $\sim$, and $\supset$. Or, leaving the most delicate case of $\supset$ out, even with just $\wedge$, $\vee$, and $\sim$. And people have felt that we have a well-defined notion of 'adopting a logic'. In fact, even now lots of the literature supposes that we have some standard procedure for 'adopting a logic'.

The logical positivists – say Carnap – thought we could adopt one logic or another, one language or another, and that a principle of tolerance should be exercised. One can adopt any kind of laws for the logical connectives one pleases ('… in logic, there are no morals', Carnap 1937, pp. 51-2).[16] Only some kind of scientific utility should make you prefer one to another. They thought that it was just a convention which logic we 'adopt' (whatever that may mean). But one is completely free to choose.

Of course, a choice of a different logic is a choice of a different language form. Nevertheless, the positivists also thought that the sort of considerations at play when we adopted a logic were different from those in the standard case of science. Against this, Quine made the revolutionary suggestion that this wasn't so; 'adopting' a logical law was no different from adopting a scientific hypothesis:

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth-values have to be redistributed over some of our statements. Re-evaluation of some statements entails re-evaluation of others, because of their logical interconnections – *the logical laws being in turn simply certain further statements of the*

[16] For example, Carnap writes: '… let any postulates and any rules of inference be chosen arbitrarily; then this choice, whatever it may be, will determine what meaning is to be assigned to the fundamental logical symbols' (Carnap 1937, p. xv). What I say in the text above is a familiar oversimplification of Carnap's and the positivists' views. Whether Quine (1936) was meant to be a direct attack on Carnap, and whether his version of conventionalism is accurately portrayed there, are interesting questions that I cannot discuss here (see Creath 1987). My thanks to Romina Birman for pointing this out.

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> *system, certain further elements of the field*. Having re-evaluated one statement we must re-evaluate some others, which may be statements logically connected with the first or may be the statements of logical connections themselves. (Quine 1951, p. 42, emphasis added)[17]

There are two things we can do. If we have a hypothesis which logically entails an observation that turns out to be false, then we must re-evaluate either our hypothesis or the logical laws used in the derivation of the observation. For example, suppose you believe that there is no table in this room and then you see a particular table. If you say, 'Nothing in this room is a table' (from which, by the law of Universal Instantiation (UI),[18] it follows that this, in this room, is *not* a table), or equivalently, 'Everything in this room is not a table', you could plead hallucination,[19] or just modify the UI law and still assert that there is no table in this room.

> Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler

[17] I am not certain what these logical laws are supposed to be. Are they that all statements of a certain form are true, as are all their consequences given certain rules? If so, and assuming, as Quine must have, the usual Tarskian theory of truth, these statements must be in a metalanguage. But then, one must postulate the same (or can they be different?) logical laws for a metalanguage. This issue will continue to a meta-metalanguage, and so on, *ad infinitum*.

Note that one would have to universally instantiate them to get any consequences, and one would have to use the Tarskian disquotation principle and *Modus Ponens*. We are going round and round in a circle.

Also, a point Padro (now Birman) (2015, ch. 4) rightly emphasizes is Quine's failure to distinguish between the statements of the logical truths and the inferential transitions that connect the rest of the statements of the system. This is evident in the passages just quoted but also when he says that 'the logical truths, at least, and no doubt many more commonplace sentences too, are germane to all topics and thus *provide connections*' (Quine 1960, pp. 12-13, emphasis added).

[18] That is, in this case, that if something holds for all physical objects in this room, it holds for any particular ones.

[19] This is an obvious possibility. Curiously, Quine doesn't consider, 'It looks as if there is a table in this room'. Surely, this is a meaningful statement, no matter how 'physicalist' one may be. Then pleading hallucination is irrelevant. We need not make much of this here. (I also won't worry about whether one can 'see' that something is a table, because we must know its function.)

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> superseded Ptolemy, or Einstein Newton, or Darwin Aristotle? (Quine 1951, p. 40)

This is Quine's famous view. What is interesting is that Putnam – more strongly, I think, than Quine himself – really takes it with dead seriousness and thinks that this has actually happened: we must revise our logic, give up not the law of excluded middle (which he holds), but the distributive law in the face of the scientific advance of quantum mechanics.

Let's forget the sophisticated case of quantum logic for a moment and consider the case I just mentioned – the law of UI. A follower of Quine, George Berry, says:

> … the distinction between the *a priori* (or analytic) and the *a posteriori* (or synthetic) cannot in general be drawn. What has passed for the one differs from what has passed for the other only in being less affected by observation, but this is merely a matter of degree. The logical rule of universal instantiation, the Pythagorean theorem and Newton's law of gravitation display in ascending order varying degrees of dependence upon the world of sensory data. Yet these three are similar in that, although none confronts that world directly, each mediates between the so-called observation sentences that do; witness the fact that each of the three principles may play its role in the single prediction of one observed position from others. Each such principle provides connective tissue invaluable in passing by inference or computation from observation to observation. Each is justified pragmatically for each is tied up with observation by being part of the overarching theory which is science as a whole, itself empirical because at its edges (to use Quine's expression) it *is* immediately responsive to observation. (Berry 1969, p. 246, emphasis in original)

All of these things — the laws of physics, the rules of logic, and geometry— are used in making predictions. We use logic in making deductions from hypotheses. For example, from this very case of Newton's law of universal gravitation — any two bodies attract each other[20] — we can then deduce that the moon and the earth attract each other, using the

[20] Of course, Newton goes on to give a famous quantitative statement of the magnitude and direction of the attraction. The geometric law cited by Berry could be relevant if these are included. We are deliberately oversimplifying here.

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law of UI. Both play a role in making this prediction. If it turns out to be correct, then one might say that both, Newton's law and the law of UI, have been well-confirmed by this particular observation.[21]

Now, Quine, following Neurath, believes we should follow a maxim of minimum mutilation. That is, if some evidence or experience calls for a revision of our total theory, one should prefer to make a small change rather than one that requires global revision. The laws of physics are well-confirmed, if they are, by particular experiments in their domain. The laws of logic, being laws of the broadest scientific field, are well-confirmed in almost every domain. Because of that, there might be a certain 'prejudice' (that's my sarcasm) to give them up last, given that they have such a great deal of confirmation in so many different domains of experience. Our reluctance to abandon logical laws unless it is really necessary is a special case of this.

The following passage from Gardner illustrates the point:

> The maxim is that when we have to choose among alternative revisions in a theory to account for unexpected observations, similarity of a new theory to the old counts heavily in its favour. One way to avoid unnecessary radical revisions is to abandon the less general beliefs used in deriving the false prediction. If we find a white raven, we abandon 'All ravens are black' rather than quantification theory, which applies to our entire world-theory and ought not to be held responsible for this failing of the biological part of it. (Gardner 1971, p. 526)[22]

[21] In fairness to Quine, I have just selected a fragment of his views. Some other parts are hard to reconcile with his long-expressed position on the revisability of logic. His view in Quine (1970, ch. 6) that any change of logic *is* a change of meaning (captured by the slogan 'change of logic, change of subject') almost reads as a recantation. (His position seems to come from his reflections on attributions of pre-logical mentality as a result of bad translation in Quine 1960.) However, he reiterates his position on revisability in the next chapter, but states that proposals to revise logic for empirical reasons violate the 'maxim of minimum mutilation'. He denies any tension between these views (Quine 1995, p. 352). My thanks to Romina Birman for pointing this out to me. I can't go into Quinean exegesis here. Putnam in his paper takes the line that (at least) it is not clearly true that a change of logic, in particular his own proposal, is a change of meaning. In any event, according to him, the issue is more relevant to linguistics than to the philosophy of logic. I will suppose that the issue about meaning is not to be discussed here.

[22] Gardner then says:

> … After examining (too briefly) the alleged quantum-theoretical utility of three-valued and nondistributive logics, Quine remarks:
>
> > The merits of the proposal may be dubious, but what is relevant just now is that such proposals have been made. Logic is in principle no less open to revision than quantum mechanics or the theory of relativity. (*Philosophy of Logic*: p. 100)
>
> Quine is certainly right in suggesting that the technical demerits of these particular proposals leave open the question of whether logic is empirical. However, it is unclear why Quine thinks

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Remark: Given the idea that our received quantification theory is simply a set of well-confirmed hypotheses, what is the role of the completeness theorem? Why was it important to prove quantification theory complete? (See also note 16 above.)

What I want to emphasize here is that in Quine's model we have a choice: if one wishes to modify one's system one can choose to modify either the particular hypotheses like 'John, a raven, is black' (or perhaps 'If John is a raven, then John is black') or the logical laws. (Does anyone really take this seriously? Does anyone *really* think that we have this choice? But anyway, here we go. We have a choice…) A maxim of conceptual conservatism, or of 'minimum mutilation', should lead us to generally prefer the alternative that doesn't change logic. If we find a white raven, we should abandon the particular hypothesis 'All ravens are black' rather than quantification theory. The law of UI is confirmed empirically by the fact that every time that we have run into a conflict like this it has turned out to be more scientifically fruitful to change biology, physics, or what have you – to change the universal statement rather than to change the law which allows us to infer a particular instance from a universal statement.[23,24] But we still have the choice.

The impression you will get from that kind of discussion is that someone would not be intellectually committed to asserting that 'This

> (as he appears to here) that the mere fact that proposals to revise logic for empirical reasons have been made, is adequate to establish that such proposals are at least coherent – i.e. to establish that logical laws are confirmed or disconfirmed through the role they play in a total scientific theory, and are therefore open to revision when the theory conflicts with experimental fact. What *does* establish this latter thesis, however, is simply that there is no intelligible alternative, as Finkelstein argues in conversation. (Gardner 1971, p. 528, emphasis in original)

As Gardner says, the mere fact that such proposals have been made doesn't show that logic is indeed open to revision: someone can just be talking nonsense by making such proposals. After all Quine wouldn't say that the mere fact that a proposal to believe things on the grounds of religious faith rather than on the grounds of scientific method has been made shows that it is open for us to do this, whether or not the particular proposal is acceptable. Similarly, for intuitionistic logic, from the fact that some of its proponents may have viewed it as a rival to classical logic, it doesn't follow that this is the correct way of viewing it.

[23] Here I am assuming that UI is all that is relevant, that is, that the universal quantifier ranges over an appropriately restricted non-empty domain. If instead we take the quantifier to range over a larger domain, so that it is a universal quantifier followed by a material conditional, *Modus Ponens* will be involved as well.

[24] Is this really true? If we note that time and time again UI in conjunction with a scientific law leads to a contradiction, we should look to the common factor, viz. UI, for revision. It would be far simpler to revise logic rather than physics, biology, geography, and so on. Moreover, quantification theory is a relatively recent invention, as opposed to the other subjects, and really known to only a few specialists.

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raven is black' just because all ravens are black. But he is committed to 'This raven is black' if, *in addition*, he accepts various logical laws, in particular: all universal statements imply their instances.[25] When he has these two things in the system, then he is committed to this raven's being black and can say that this law is well-confirmed, or being shown to be fruitful in the empirical predictions for which it is needed. And when he sees that this raven isn't black, and he accepts that and doesn't believe it's a hallucination, he has to give up one or the other of these.

Now, regardless of the point of re-evaluation, I want to consider what I take to be the main point of Quine's passages: are the laws of logic '*simply certain further statements of the system, certain further elements of the field*'? This is why – at least in Quine's picture – it is then argued that they are supposed to be up for re-evaluation. (Perhaps these issues are independent: someone might hold that the logical laws are empirically revisable even if they are *not* just certain statements of the system. But this would require a different formulation of the argument.)

Can we 'adopt', rather than a non-standard logic, the logic that we have, which includes the law of UI? The basic objection to this has already been given by Quine himself. He summarizes it as follows:[26]

> The difficulty is the vicious regress, familiar from Lewis Carroll, which I have elaborated elsewhere. Briefly the point is that the logical truths, being infinite in number, must be given by general conventions rather than singly; and logic is needed then to begin with, in the metatheory, in order to apply the general conventions to individual cases. (Quine 1954, p. 115)

Quine re-states here an argument which he was pleased to give against the Vienna Circle, and against Carnap in particular (see footnote 15), but not against himself. He takes this argument to be the point of Carroll (1895). The view Quine is questioning is that modern logic has shown that logic is analytic (this term is used once) or conventional (the term

I don't see that we really employ a maxim of minimum mutilation. Sometimes, if we find ourselves making lots of piecemeal revisions, we look instead to the well-entrenched theory. Also, in the Copernican-Ptolemaic case, adding another epicycle would amount to a smaller change, but we think nonetheless that Copernicus was right. Perhaps we see more epicycles looming in the future, but then it would be in accord with Quine's simplicity maxim to deny UI. Another puzzling feature of all this, at least to me, is the status of the maxim itself. Is it also subject to revision? Is it empirical?

[25] Note how Quine's failure to distinguish between the statements of the truths of logic and the inferences themselves is also relevant here.

[26] The argument first appears in Quine (1936, pp. 351-2).

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usually used in Quine 1936). And the basic problem is this: *if* the logical truths are mere hypotheses, just like anything else, and one can 'adopt' them as one will, how, unless one has a logic in advance, can one possibly deduce anything from them?

If one believes the Quinean doctrine, one would think that the law of UI is just a statement, part of our system of beliefs, like any other. We have chosen to 'adopt' it. Its fertility is shown by the fact that it has led to fruitful results. I think, on the contrary, that understood in this way it has had *no* utility whatsoever.

To see this, let's try to think of someone – forgetting any questions about whether he can really understand the concept of 'all' given his condition – who somehow just doesn't see that from a universal statement each instance follows. *But* he is quite willing to accept my authority on these issues – at least, to try out or 'adopt', or use provisionally, any hypotheses that I give him. So, I say to him, 'Consider the hypothesis that from each universal statement, each instance follows'. Now, before being told this, he believed me when I said that all ravens are black, but he was unable to infer that *this* raven, which is locked in a dark room where he can't see it, is therefore black. He doesn't see that this follows and that this is actually true. So, I say to him, 'Oh, you don't see that? Well, let me tell you, from every universal statement each instance follows'. He says, 'O.K., yes. I believe you'. Now I say to him, '"All ravens are black" is a universal statement, and "This raven is black" is an instance. Since all universal statements imply their instances, this particular universal statement, that all ravens are black, implies this particular instance'. He responds: 'Well, hmm, I'm *not entirely sure*. I don't really think that I've got to accept *that*'.

This is the problem. If he did not already reason in accordance with the pattern of inference we call 'Universal Instantiation', telling him that it was true would do him no good: he couldn't 'adopt' it as a hypothesis, he couldn't use it as an auxiliary to derive further statements. If he was not able to make the simple inference, 'All ravens are black, therefore, this raven is black', then giving him some 'super premise' like 'Every universal statement implies each instance' as another premise won't help him either.

This point is indeed vaguely familiar from Carroll, but it hasn't really been appreciated in some of the literature I have 'instantiated' (joke; I mean the particular papers I have read). Quine, the one person who has famously deployed this argument in the philosophical literature, nevertheless says that the logical laws are 'simply certain further statements of the system, certain further elements of the field', just like

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any others. If one wishes to modify one's system one can choose to modify either the particular hypotheses like 'All ravens are black' or the logical laws.

But why? If he wasn't committed to it from the statement 'All ravens are black' alone, why did he become committed to it from these two together? After all, if 'All ravens are black' doesn't *already* commit one, without anything extra like 'adopting' a logic or accepting a logical law, to 'This raven is black', why does 'All universal statements imply their instances' commit one to 'This universal statement implies this particular instance'?

Even if the law of UI were necessary to lead up to this conclusion, it wouldn't help much. If we understand it as a hypothesis or belief that we 'adopt' in order to (indirectly) determine its empirical impact, we should conclude that its scientific fertility is *zero*. It will not lead to a single prediction. If we are free to assert, 'All ravens are black', and deny, 'This raven is black', given that we don't accept this law, then we are free to assert, 'All ravens are black', and deny, 'This raven is black', even if we *do* accept this law. All we have to do is say that this is another universal statement that doesn't imply all its instances: 'This universal statement doesn't imply various particular instances of this either'. You point out, 'I said, "Look, all universal statements imply this"'. He then responds, unconvinced: 'Yes, yes...'

Some might think, 'Ah, the point of this is that we shouldn't adopt a statement, as Quine says – we really mean to adopt a rule'. But what does 'rule' mean? If it is an imperative that I tell this guy – 'Always infer each instance of a universal statement from a universal statement' – that will not help him either.[27] Suppose he agrees to obey. I, of course, will not regard him as having really obeyed it if he doesn't infer from universal statements their instances. But if we are 'adopting' a so-called neutral standpoint from which it is supposed to be up for grabs what follows until we are told what rule to 'adopt', then one can quite well say, 'All right, I will always infer each instance of a universal statement from the universal statement itself. I don't see that this means, though, that I must infer *this* particular instance of *this* universal statement'. Without begging any questions, I don't see that this follows either. After all, if the inference wasn't obvious to him in standard declarative logic it will not

[27] Actually, as Romina Birman (2023, p. 11, and Padro (now Birman) 2015, pp. 57ff.) points out, such an imperative is ridiculous – it could not be fulfilled if its recipient has anything else to do but drawing countless instances of some universal statement. There must be a better formulation that would really work.

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be obvious in imperative logic either. So, the 'adoption' of something that he will assent to – of this statement as a rule – will not help him either.

Of course, I won't believe that he's obeying a rule if he doesn't do this in particular cases. That's because, from my point of view, logic with a capital 'L' is not a logic that I may 'adopt' or in which I may dictate that this inference follows in imperative logic. Or, if one were trying to take the Quinean view – the alternative view – one would have to say, 'I hold a logic different from him and it's because I do so that I conclude that he has not followed my rule'. But if I held a different logic, I may *not* conclude that he did not follow my rule – that is, if one is supposed to be 'adopting' a neutral stance between a logic which allows UI and one which rejects it.

Suppose this man were even so perverse that he had, or used in practice, a rule which Kit Fine called in conversation 'perverse instantiation' – let's follow this terminology. From each universal statement he infers the negation of each instance. I might try to correct him and say, 'Look, every instance of this perverse inference pattern is invalid'. I then find him proceeding to infer according to this kind of pattern as before. I say, 'No, didn't I tell you that all inferences according to that pattern are invalid, and isn't this an inference according to that pattern?' He will agree and say, 'Therefore, this pattern of inference is not invalid – that is to say, is valid'. If this is up for grabs, one hasn't gotten very far in correcting him by telling him this.

Lewis Carroll, in his famous essay, 'What the Tortoise Said to Achilles', used *Modus Ponens* for a similar problem. If someone cannot see the inference from p and if p then q, to q, you will not help him by telling him: 'Look, the following is correct: if p and if p then q are true, then q is true'. This is because if you just add this on as an extra premise, one will still have to use *Modus Ponens* to get anywhere. (Also, the rule of Adjunction, discussed below, is used to get the conjunction of the two premises: p and if p then q.)

Many people have thought, erroneously, that this was just a problem with a rule of inference as opposed to a statement. From this some might conclude that *Modus Ponens* is the only case in which this problem can arise. After all, there are perfectly rich formal systems in which *Modus Ponens* is the only primitive rule of inference. So, why isn't this the only case where this problem arises? Well, in the case of UI the answer should be clear. The axioms in such formal systems are infinite in number even if they have only *Modus Ponens* as a formal rule of inference. Now, if one could tell a person each instance of this separately, this would be

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fine. But we can't do that, of course, because they are infinite in number. So, if you're really telling someone to 'adopt' this system, you would have to tell him, 'Accept all instances of this schema', or 'All instances of this schema are true'. And to conclude that a particular instance of this schema is true, he must already use UI in practice. He must conclude, 'If all instances of this schema are true, this particular instance of this schema is true'. Otherwise, he will not get anywhere.

This is an example of the kind of illusion that we get into by a mechanical consideration of formal systems. It's because of these mechanical considerations that someone might say, 'I accept Lewis Carroll's point as it applies to *Modus Ponens* alone'. She would be wrong. In fact, it applies to a number of cases. One of the most interesting cases to me was the law of non-contradiction, because some of the religious writers that I've talked about thought that we can modify logic actually to the extent even of 'adopting' contradictions as true.[28] We need only hold that the law of non-contradiction doesn't always apply in this domain. And it is not just these religious writers. Engels, for instance, defending Hegelian logic, gives a different kind of argument:

> Even lower mathematics teems with contradictions. It is for example a contradiction that a root of A should be a power of A, and yet $A^{1/2} = \sqrt{A}$. It is a contradiction that a negative quantity should be the square of anything, for every negative quantity multiplied by itself gives a positive square. The square root of minus one is therefore not only a contradiction, but even an absurd contradiction, a real absurdity. And yet $\sqrt{-1}$ is in many cases a necessary result of correct mathematical operations. Furthermore, where would mathematics — lower or higher — be, if it were prohibited from operation with $\sqrt{-1}$? (Engels 1877, Chapter 12, 'Dialectics. Quantity and Quality', p. 154)

So, the whole point is that a statement and its negation can both be true. Now, imagine someone says to me, 'You who believe the law of non-contradiction are only making an assumption. I wish to consider the opposite assumption'. Can I be making an assumption? Suppose a follower of Engels contradicted himself a lot – asserts both p and not-p – because he believes that contradictions can be true. I want to get him out of this. He is, once again, willing to modify his beliefs according to what I tell him. I say, 'Look, I hold the law of non-contradiction: a statement

[28] This might not be so shocking now as it was then. Graham Priest (and also the late Richard Sylvan) and their advocacy of the acceptability of contradictory theories were unknown to me then, though David Lewis, who used to spend much time in Australia, approached me after a seminar in which I was discussing these issues and told me about their point of view.

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and its negation can't both be true'. He agrees to believe it, but goes on asserting p and asserting not-p. So I ask, 'Didn't you listen to what I told you?' And he replies, 'Yes, and I believed you. But, as usual, you only see *half the coin*. I agree that "p and not-p" is false. But I also think that it is *true*. That is just another one of the contradictions revealed by the dialectic'. You can tell him, 'These two statements are contradictory too, and you can't accept them', but he will go onwards, and you cannot get him out of this.

One really doesn't have to abandon the law of non-contradiction if one wishes to contradict oneself. You can make the law of non-contradiction another contradiction – the very deepest contradiction of all: just because *it is true* that a statement cannot be true and false, it does not mean that this principle is *not also false*. So, it seems the law of non-contradiction could not be adopted as a rule either – 'Never hold a statement to be both true and false' – because perhaps one could both obey and not obey it, if this kind of issue were up for grabs.[29]

Another interesting case is: $p, q \Rightarrow p \wedge q$ (Adjunction). Friedman and Glymour (1972) propose to abandon this principle in quantum logic (Putnam 1981 also makes this proposal; this seems to depend on withdrawing his original 'realist' ideas). Suppose you tell it to someone in the form of the rule: 'If you accept p and you accept q, you must accept $p \wedge q$'. He then agrees that he has accepted p. He also agrees that he has accepted q. You say to him, 'My rule tells you what to do. You've accepted p and accepted q, right?' (Hesitantly) '*Hmm*…' (I'll leave it to the reader to figure out the difficulty.)

The point is that logic, even if one tries to throw intuitions to the wind, cannot be just like geometry because one cannot adopt the logical laws as hypotheses and draw the consequences. You need logic in order to draw these consequences. There could be no neutral ground in which to discuss the drawing of consequences independently of logic itself. This is the basic point that I want to make. But, it will be said, maybe this doesn't apply to all of logic. Maybe there is some more basic ur-logic for which these kinds of arguments apply. Maybe it doesn't apply, for example, to the distributive law. And maybe our argument only applies to such cases.

[29] As I said, I originally gave this material in ignorance of the ideas of Priest and Sylvan. But, in fact, Priest (1979) does accept the law of non-contradiction and its negation. I don't think he says that this is the deepest contradiction of all.

Priest also says that once we admit that the object language theory may not be consistent, there is no reason to suppose that the meta-theory is consistent either. In some ways he is quite consistent with his own views!

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Such a reply doesn't really get the point. I don't really mean that we 'adopt' as basic just those things to which we can figure out that this argument applies. What I mean is this: you can't undermine intuitive reasoning in the case of logic and try to get everything on a much more rigorous basis. One has to just think, not in terms of some formal set of postulates but intuitively, that is, one has to *reason*. One can't just 'adopt' a formal system independently of any reasoning about it because if one tried to do so, one wouldn't understand the directions for setting up the system itself. And so, any comparison of logic to geometry which would say that in the case of logic, as in the supposed case of geometry, intuition can be thrown to the dogs – that is, any reasoning outside the system of postulates can be thrown to the dogs – must be wrong. One can only reason as we always did, independently of any special set of rules called 'logic', in setting up a formal system or in doing anything else. And if proof by cases was part of our intuitive apparatus, there is no analogy to what is being alleged about geometry, which says that somehow intuitive reasoning should not be respected. And that is really the point.

Before we move on, I want to say what I'm *not* saying.

(i) I said that one can't give up intuition. One can only reason intuitively. But 'intuitively' is now often a pejorative term: 'What's intuition? That sounds more like faith'. On the contrary, I mean something like reason. One can only reason *by reasoning*. One cannot reason by setting up a formal system and then deciding to reason within it. Because if one had no way of reasoning before and tried to hold that in suspense, then one could not understand this alleged setting up of a formal system or follow its rules.

(ii) I am *not* saying, however, that the laws of logic can never be changed. I don't know what they mean by that because I don't know what it means to 'adopt' a logic. We didn't set up the laws of logic that we have. We recognize that certain principles of inference hold, but we did not *adopt* these. How could we have adopted them? If we tried to adopt them, that would've done no good.

In fact, two kinds of changes are clearly possible. Once logicians have actually formulated what principles hold in logic, they *can* sometimes turn out to be wrong. They are wrong because of an intuitive argument, not because a different formal system was proposed and then 'adopted'. The most famous case, and the one I think least open to dispute, is the case of the traditional logic, Aristotelean logic, which accepted the inference from 'All *A*s are *B*s' to 'Some *A*s are *B*s', without realizing

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that this leads into trouble if empty terms are allowed. A law of logic was accepted for many, many years, then it was later seen to be a mistake – not because someone said, 'Let's adopt a different formal system' (whatever that means) but because a mistake was noted, namely, that we overlooked the possibility that the term '*A*' is empty.[30]

Here we have a famous case where an inference pattern which used to be universally accepted is now widely disputed or widely thought to be false. This was not because they said, 'That was one logic. Let's adopt another', but because of intuitive thinking. It was that this principle of inference, which used to be universally accepted, we now argue, giving an intuitive argument – that is, an argument independent of adopting a formal system – is, in fact, fallacious.

Another famous case is intuitionistic logic. I will just have to say rather dogmatically what I think about this case. Intuitionists, I think, are not really proposing to modify the laws of logic with the classical connectives.[31] They propose new connectives, more appropriate according to them for mathematics, which satisfy different laws. One sees, in virtue of the interpretation that they gave to their connectives and quantifiers, that a different set of laws is satisfied by these distinct concepts. But one does not just postulate that a different logic should be 'adopted'. I think, by the way, that they do not even propose to reject the classical connectives as meaningless. They just think that these connectives, as applied to mathematical statements, would lead outside the domain of mathematics. That is, the classical negation of a mathematical statement won't always be a mathematical statement.[32] I do not think that this famous case is a case where a different logic was 'adopted', whatever that may mean.

[30] Interpretations have been proposed that will verify all the traditional laws, the immediate inferences, the square of opposition, the traditional syllogisms, and 'All *A*s are *A*s'. But these are plainly not the intended interpretation. As to the intended interpretation, one might try to impose a general restriction, analogous to the one in first order logic that the domain is non-empty. If one wished to justify the syllogism and all the relevant rules that way, one would have to propose the restriction that all the terms are non-empty and have non-empty complements (relative to the universe of discourse). This would be severely restrictive.

[31] Though, admittedly, Brouwer did sometimes sound that way, or rather, as if he had discovered fallacies in the accepted laws, just as in the case of the traditional syllogistic mentioned above.

[32] Actually, classical negation sometimes occurs informally in intuitionistic mathematics. Intuitionistic negation is called 'absurdity', 'it is absurd that'. But sometimes one wishes to say that a statement *A* 'has not been proved'. Here the negation is classical (and temporal, since maybe it will be proved eventually). I have myself published an alternative interpretation of intuitionistic logic in Kripke (1965).

I should add that some people, including the present writer, Gödel, and Kreisel, think both that intuitionistic mathematics makes sense and that classical mathematics does. See also Kripke (2019) for an interpretation of intuitionistic mathematics that definitely allows classical mathematics to be valid.

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One may always, of course, invent new connectives which are similar in meaning to the old ∨ and ∧ (or anyway, similar in the laws they satisfy), but satisfy somewhat different laws because they have a somewhat different interpretation. That should be uncontroversial. It's not that we set up the different laws and then, as the literature often says, 'deduce' that they must have a different interpretation – as Carnap, and sometimes Quine, would seem to argue. But rather, it's the other way around. One sets up a different interpretation and then sees that in virtue of this non-standard interpretation, non-standard laws follow. So one can correct a mistake; sometimes, even something that seems to be quite self-evident can be based on overlooking something. This is certainly true of a complicated principle of logic, and it could be true of a simple one, as the case of Aristotelian logic shows.

Still, whenever someone gives a complicated mathematical proof which turns out to contain a fallacy, in a sense something was accepted as a truth of logic and is later rejected. This would amount to (for example) accepting that the axioms of Bernays-Gödel set theory imply the statement in question, when in fact they do not. That's a purely logical assertion. When the fallacy is found, it is realized that this logical assertion is dubious, or maybe actually false, when someone refutes it by a counterexample.

So in a complicated case one can certainly be wrong about what's a truth of logic. But even in a case where it seems self-evident, one can turn out to be wrong later by further intuitive reasoning, as in the case of the Aristotelian logic: a simple case was overlooked there, namely, the case where the subject term is empty in 'All *A*s are *B*s'.

These cases shouldn't be controversial; they shouldn't undermine the notion of self-evidence. The fact that something can seem to be self-evident and later turn out to be wrong shouldn't undermine our using this as a principle of seeing that things are true any more than the fact that something may seem to be supported by experiment and then later turn out not to be so well-supported by experiment should undermine our using *being supported by experiment* as a justification for accepting something.

Let me apply this to the Putnam case. Lots of the confidence that people have had that there are alternative logics comes from the existence of alternative formal systems. Putnam refers to a formal system called 'quantum logic'. What's it supposed to be? What he does say is that the logic is to be read off the Hilbert space. Now that doesn't make a great deal of sense if you don't know about Hilbert space, but the basic idea is this. Suppose one thinks of classical logic as corresponding to

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certain set-theoretic operations on subsets of a set: $\vee$ is union, $\wedge$ is intersection, $\sim$ is complementation, and so on.[33] Then there are certain more complicated operations on subspaces of the Hilbert space which have some of the properties of $\vee$ and $\wedge$ and so on – they form a lattice, whatever that means, in a technical sense. $\wedge$ is just the intersection, again, of two subspaces, but $\vee$ is the smallest subspace containing the two subspaces, which is not the set theoretic union but generally something bigger. Negation corresponds to the orthocomplement of a subspace, that is, the space generated by those vectors that are orthogonal to the subspace.

It turns out that many of the classical laws are satisfied, for example, that $A \vee B$ is equivalent to $(A \vee B) \vee A$, that $\sim\sim A$ is equivalent to A, and that the law of excluded middle holds. The De Morgan laws, such as the equivalence of $\sim(A \vee B)$ with $\sim A \wedge \sim B$, also hold. But the distributive law fails. This is what is well known as the notion of a non-distributive lattice, with a complement.[34]

Propositions correspond to subspaces of Hilbert space. Any proposition corresponding to the whole space can be called logically true. One proposition A implies another B if and only if the corresponding spaces A' and B' are such that A' is contained in B'. The smallest element (the empty space) is logically false.

Let us distinguish various ideas. There is a lattice of subspaces of Hilbert space, or indeed (as in footnote 33) of any vector space with a suitable inner product. Nothing is wrong with this. Nor is there anything objectionable about introducing *new* connectives corresponding to such spaces and interpreting them in terms of quantum mechanics. This obviously presupposes the intelligibility of quantum mechanics and cannot be used to define how it is to be understood. The new connectives, though having certain analogies to the traditional ones, can be intelligible side by side with the classical ones. But this is not Putnam's position. His view is that for empirical reasons we have discarded the traditional laws in favour of a non-classical logic which is incompatible with them.

[33] But see Maudlin (2005, p. 160), first paragraph, for an example where set theoretic union does not completely correspond to classical disjunction.

[34] The case of quantum Hilbert space is a bit more complicated because the relevant scalars are complex numbers, and the inner product must be evaluated somewhat differently from an ordinary vector space with an inner product (say over the reals as scalars). These complications do not seriously affect the discussion. One can think of everything more simply if, following Maudlin (2005), one considers ordinary Euclidean 3-space.

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Part of the set of specifications is that it is all based on classical mathematics, which uses classical logic. And that is of course what is supposed to be given up as false. We're not supposed to be introducing new connectives. We're supposed to be discovering a mistake in the old classical logic. How can I understand this specification? I first have to know what the Hilbert space based on this new logic is. And given that I understand that, perhaps I then can see what principles hold. But it revolves in a circle, because if I don't know the new logic, I won't know what will hold in the new kind of Hilbert space.

Perhaps the suggestion is supposed to be that in mathematics, classical logic will be undisturbed. Putnam has given only a system of propositional calculus, specified in terms of subspaces of Hilbert space, but he hasn't really gone much further. We don't know how this will fit into a system of mathematics and physics. He really needs a much bigger formal system.

Let's ignore the fact that he really hasn't *interpreted* this formal system, which I think is very important,[35] and suppose that we can just think purely syntactically. I still don't know what his formal system is, but I will play the following game: I will accept the statements (1) – (6), which he says are even logical truths.[36] I will try never to use the distributive law in my reasoning.[37] I don't know how this answers to Putnam's intentions, because we don't know if he intends to capture the rest of mathematics. But here is a theorem (in that sense of 'theorem'):

$$2 \cdot 2 \geq 5.$$

(This is a somewhat startling result.)

The usual mathematical definition of multiplication is this: suppose we have two sets with two elements. Then the cardinality of their product is the cardinality of the Cartesian product of the two sets – that is, the number of ordered pairs $<x, y>$, where x comes from the first set and

[35] He gives an interpretation in Hilbert space, but not an intuitive interpretation. I think that's quite important, but perhaps he doesn't.

[36] That is, when you interpret them on the Hilbert space interpretation, they all come out to correspond to 'the whole space', whatever that means. That's what he calls a logical truth. I don't think that's quite the right terminology to use, but anyway they are laws of physics at least, supposedly.

[37] The distributive law is really proof by cases or disjunction elimination, allowing the disjuncts to be considered separately, even when one has other side premises.

In Putnam's proposal, we can't say, '$q \vee r$ is true. If q is true, something is true. If r is true, the same thing is true', and use in the reasoning any other premise p. We can use only q and r alone, as we're rejecting the distributive law. But even according to Putnam it is O.K. to use premise p, *provided it's a logical law that corresponds to the whole space*. The reasoning will be fallacious if it allows the use of a side premise, such as $A = 1$. This is so even though one may 'realistically' entertain that the side premise may really be true.

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y comes from the second set. So where x comes from $\{a, b\}$ and y comes from $\{c, d\}$, where $\{a, b\}$ and $\{c, d\}$ are our two-element sets, we want to consider how many ordered pairs there are. So the classical arithmetician says, 'There are four', namely:

$$< a, c >, \quad < b, c >, \quad < a, d >, \quad < b, d >.$$

But we can all see the fallacy in any conclusion that these are the only pairs. Since x comes from the first set, then we have: $(x = a \vee x = b)$. Since y comes from the second set, then we have: $(y = c \vee y = d)$. Therefore, it is concluded:

$$(x = a \wedge y = c) \vee (x = b \wedge y = c) \vee (x = a \wedge y = d) \vee (x = b \wedge y = d).$$

But this, of course, is a fallacy – that's the distributive law. We can in fact see from what Putnam has already told us that not only is the classical arithmetician's proof fallacious, his conclusion is in fact false. Let's replace a, b, c, d respectively with the numbers: 1, 2, 1, 2. Then there are indeed four pairs: <1, 1>, <1, 2>, <2, 1>, <2, 2>. This we will not contest, but I claim that there is a fifth pair, <A, B>, where these are the two quantities mentioned by Putnam. Remember that Putnam does not think that these are funny pseudo-numbers. The whole idea is that A was already definitely one of the numbers 1 and 2, and B was already definitely one of the numbers 1 and 2. So A is certainly in the first set [$\{a, b\} = \{1,2\}$] because A is equal either to 1 or to 2. B is certainly in the second set [$\{c, d\} = \{1, 2\}$] because B is equal either to 1 or to 2, though we may not have measured which. So, the pair <A, B> is in our Cartesian product. But certainly we cannot say that <A, B> equals <1, 1> because that would mean that A equals 1 and B equals 1 and that contradicts (3). Also, <A, B> does not equal <1, 2> because it is false that A equals 1 and B equals 2. And <A, B> does not equal <2, 1> by (4). <A, B> does not equal <2, 2> by (5). So here is a fifth and hitherto overlooked, I might say, ordered pair in the Cartesian product of these two finite sets. Therefore, we have that 2 x 2 is at least 5.[38]

Let's swallow this for the moment. Who knows if this is so much worse than what we were asked to swallow in the beginning. How can we possibly even begin to proceed now? We were supposed to read the logic off the Hilbert space, but if even elementary arithmetic becomes questionable, how can we be sure what the mathematics of something more complex, such as Hilbert space, would be? What we can do is set up a formal system which is motivated by the fact that it's supposed

[38] This argument is mentioned in Dunn (1980) and Williamson (2018), footnote 18.

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to hold in classical Hilbert space. (By 'classical Hilbert space' I mean Hilbert space set up using ordinary logic and mathematics.) It's motivated by that, but we don't mention the motivation afterwards – we 'kick away the ladder'. We accept just these formal rules.

The whole illusion that has created all this talk about adopting a logic, and so on – as if one can do it – basically comes from this: we're all taught in logic textbooks that when you have a formal system you do not need to reason. A *machine* can do it. A moron can follow these directions. All you have to be is programmed, and so on. This, of course, is true. A machine *can* do it. All you have to be is programmed. Then one thinks that one has something really solid and independent of any reasoning: just *do this*. But the trouble is that, after all, a given machine may or may not be correctly programmed to follow out this formal system.

First, it may blow a fuse. Surely, if a machine set up to do *Principia Mathematica* concludes, let's say, that the distributive law fails in *Principia Mathematica*'s logic, one would think that something has gone wrong with either the programming or with (say) the fuse, not that it doesn't hold in *Principia Mathematica*. Why? Because if we have this concrete object in front of us, our belief that it produces exactly the same results that would hold in *Principia Mathematica* depends on (*a*) *a mathematical proof* that any abstract Turing machine so programmed will do so (one that uses both logic and mathematics); (*b*) *a physical argument* that this particular object, built up in this physical way, will behave like this Turing machine program (that depends on our beliefs in the laws of physics); and (c) *an engineering hope* that, in fact, the machine is well enough set-up that it won't blow a fuse at this point. All of these are very special pieces of knowledge. And one cannot, in abstraction of any knowledge of these things, have any confidence that this particular machine represents this particular formalism. It is a great illusion to think otherwise.[39]

If someone is not familiar with UI, he will therefore not understand what formal system as a result contains all instances of a particular schema, and therefore contains the particular instances of that schema. He will not understand the directions unless he can use logic in his metatheory, allowing him to reason about the rules of his formalism.

Let me not consider any kind of complicated case. Let me first consider this babyish little formal system. There are no connectives, let

[39] See my discussion in Kripke (1982, pp. 32ff. and footnote 24).

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alone quantifiers. There are only three well-formed formulae: p_1, p_2, and p_3. The first two of these are axioms. The third may or may not be a theorem. It's not an axiom. But there is the following rule of inference:

From any two statements p_i and p_j infer p_3 unless either (*a*) $i = j = 1$, or (*b*) $i = 1$ and $j = 2$, or (*c*) $i = 2$ and $j = 1$, or (*d*) $i = j = 2$.

This rule applies except in four cases. Now classically we would argue as follows: p_3 is not a theorem of this formal system because the only axioms are p_1 and p_2. So, what could the proof be? The only axioms are p_1 and p_2, so either it would be p_1 alone, but that's excluded, or it would be p_1 and p_2, but that's excluded, or it would be p_2 alone, but that's excluded.

Ah, but Putnam can say, 'No, there *is* a valid proof of p_3 in this uninterpreted formal system'. I could write it in three steps:

(i) p_A
(ii) p_B
(iii) p_3

The first step is O.K. because p_A is an axiom. This is because A equals either 1 or 2, and so it is either p_1 or p_2, but I don't know which. And the second is O.K., because once again $B = 1$ or $B = 2$, but we don't know which. But it's an axiom, because to be an axiom is to be p_1 or p_2. p_3 follows because it'll be O.K. unless this pair of premises comes under one of these four cases. But we know that, for example, we don't have the case where i and j both = 1 because of (3). We know that we don't have the case $i = 1$ and $j = 2$ because of (6), and so on. And so we know that we don't have any of these cases. So p_3 is a theorem in this formal system.

So, what's the moral? Even if one wishes to talk about uninterpreted formal systems, what one says is or is not provable in a formal system is not independent of logic, or probably, even of mathematics. Our reasoning about the formal system itself *uses* logic. The fact that we think p_3 isn't provable *uses* the distributive law, or proof by cases. We use it in particular to see what proofs there are, even in this baby formal system where all proofs must be very short. We naturally argue that there are only four possible proofs containing the axioms, since the only ones are p_1 and p_2, and hence that p_3 is unprovable. But this is not independent of what reasoning we might accept in the metatheory.

You would have to set up a meta-system which says that you can make such comparisons but you won't even know what the meta-system is without using logic – you then have to set up a *meta*-meta-system to be sure of what *that* is. But instead of getting into this dizzying game, let

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me say, in fact, you're not right to take what is provable in a formal system to be independent of the logic and mathematics used to describe it.

An interesting system is the very system which, presumably, is supposed to be the rival to Putnam's logic, namely, the classical propositional logic of Frege and of *Principia Mathematica*. That's what he's setting it up against – ordinary, two-valued, propositional logic. And Putnam agrees. Putnam thinks, in fact, that quantum logic explains why classical logic was true in our ordinary life and not true of the electron.[40] Now, I don't really know what he means, but it's rather dubious because if $2 \cdot 2 \geq 5$ then this isn't quite an ordinary life. But let's waive all that. He wants to claim that, just as Euclidean geometry is approximately true for medium-sized objects and around here and so on, so classical logic is approximately true. The comparison between the two is made on empirical grounds.

Could classical logic even be inconsistent? But no one, of course, who has read a logic book thinks so. We have a proof in a logic book that it's consistent. How does this go? It generally says that all of the theorems are truth-functional tautologies. What does that mean? Well, it means that to the atomic statements, you assign any one of the values T or F, and then, we evaluate the compound formulae by truth-tables. For example, the negation of a statement has the opposite truth-value from the statement itself. The conjunction is true if and only if each conjunct is true. (You've all heard that.) Now the point is that each axiom is always a tautology – that is, it comes out true no matter what values you assign to the atomic statements. And this property is preserved by the rules of inference. So we say all theorems get the value T, always. But then a contradiction certainly cannot be deduced because if we could

[40] Putnam himself refers to the '*approximate* validity of *classical* logic "in the large"…' (Putnam 1968, p. 184, emphasis in original). What does this mean? Maybe a 99% chance that the conclusion will be true if the premises are!

Elsewhere, he says that 'for *macroscopic* propositions the lattice is distributive; so we may keep the classical logic … for these cases. But … for *microscopic* propositions … we will be in trouble …' (Putnam 1968, p. 196, emphasis in original). I find this dichotomy between the microscopic and the macroscopic puzzling since, as Putnam himself stresses, in an earlier paper, 'It should be observed that for all its fanciful nature Schrödinger's cat involves a physical situation that arises very often, in fact one that arises in all quantum mechanical experiments. Almost every experiment in which quantum mechanics is employed is one in which some micro-cosmic uncertainty is amplified so as to affect something detectable by us human beings – and hence detectable at the macro level' (Putnam 1965, p. 152).

Perhaps the statement is that except in certain special experimental situations, classical logic is valid in our ordinary life. Putnam refers to a 'Boolean lattice' (presumably a sublattice of the Hilbert space lattice). But how do we characterize this sublattice? And note the 'inconsistency proof' for classical propositional logic below.

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deduce ($p \wedge \sim p$), well this is far from a tautology. In fact, it always has the value F. This formal property shows the classical propositional logic to be consistent.

'*But is this proof really valid*? Perhaps we have made a mistake'. First, we want to verify that every axiom is a tautology. We can take an axiom like, say, $p \vee \sim p$. The verification will be O.K.: if p has the value T, then $\sim p$ has F and the disjunction gets T. If p has the value F this gets T and the disjunction still gets T. That's quite O.K. But now, what about a statement (and there always are theorems of this form) with *two* schematic atomic letters? Then we consider all the possible ways of assigning T and F to p and q. There are, it is said, just *four* of them. But here, as the acute reader correctly notices, the classical logician has made a mistake. Not because p gets T or F and q gets T or F – rather, because that does not imply that either p and q both get T or p gets T and q gets F or … and so on. That is to use the distributive law.

We don't have to guess what Putman really thinks about this matter. In conversation with Michael Dummett, who thought that a realist view was characterized by an acceptance of classical logic, Putnam pointed out that the assumption that classical logic would be verified by the usual truth tables itself depends on the distributive law. So, one might be a realist in the sense that every statement is definitely true or false, but that does not commit one to accepting the classical truth tables as representing all the possibilities (see Dummett 1976, pp. 274-5). If one has a compound statement involving two atomic components, p and q, we cannot assume that the usual four-rowed tables represent all the possibilities. To assume this is to assume the distributive law. So on his view classical logicians made a mistake when they assumed that the distributive law and comparable laws were truth-functional tautologies. The reasoning is circular.

So we can observe that on Putnam's view the usual proof that the theorems of classical propositional logic are precisely the truth-functional tautologies is fallacious. And the usual corollary, that the system is formally consistent, has not really been proved.

Now let me prove an even stronger result:

The classical propositional calculus is formally inconsistent.[41]

[41] I could also, by the same method (but a little more complicated), show intuitionist logic to be formally inconsistent. Since classical logic can be embedded in intuitionistic logic, the latter will be formally inconsistent as well. This practically shows just about any propositional calculus in the literature to be formally inconsistent (say modal systems that already contain one of those two, or the relevance logic systems).

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The following schema is provable in *Principia Mathematica*:

$$(p \wedge q) \vee (p \wedge \sim q) \vee (\sim p \wedge q) \vee (\sim p \wedge \sim q)$$

It uses the distributive law and the law of excluded middle. We also have a rule of substitution, that for p and q we can substitute any formulae and get a valid statement. Let r be any statement.

For p I will substitute this messy thing:

$$\sim_{A-1}(r \supset r)$$

This prefix, $\sim_{A-1}$, is applied to the material conditional, $(r \supset r)$, or any other favourite theorem of classical propositional logic. Now what do I mean by $\sim_{A-1}$? I mean the statement $(r \supset r)$ is prefixed by the negation sign $A - 1$ times. Since we in fact know that A is either equal to 1 or 2, we don't put the negation sign at all if $A = 1$, and we put just *one* negation sign if $A = 2$.

Similarly for q, we will substitute, $\sim_{B-1}(r \supset r)$. The remarks about the insertion of a negation sign are the same. Then, as before, there are four disjuncts. One will get a contradiction if one can prove the negation of each disjunct. Although I use proof by cases here, there are no side premises.

Case 1

Let's take the first disjunct, namely, $p \wedge q$, that is, after the substitution is made:

$$\sim_{A-1}(r \supset r) \wedge \sim_{B-1}(r \supset r)$$

Remember the statement (3) above: it is not the case that $A = 1$ and $B = 1$. Then by the usual De Morgan law (valid in Putnam's system) either A does not equal 1 or B does not equal 1. Since $A \neq 1$ or $B \neq 1$, it follows that $(A - 1) \neq 0$ or $(B - 1) \neq 0$. In either case a negation sign precedes $(r \supset r)$ rendering this disjunct refutable in classical propositional logic.

Case 2

What about $(p \wedge \sim q)$ (in other words, or substituting, $\sim_{A-1}(r \supset r) \wedge \sim_{B-1} \sim (r \supset r)$)? By (6) and De Morgan again, $A \neq 1$ or $B \neq 2$. So $(A - 1) \neq 0$ or $B \neq 2$. If $(A - 1) \neq 0$ the first conjunct is refutable. If $B \neq 2$, $B = 1$ and the second conjunct is refutable. So in either case the conjunction is refutable.

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Case 3

This case is obviously symmetrical with the previous case, where we just use $A = 1$ or $B = 2$.

Case 4

What about the final case, $\sim p \wedge \sim q$, or actually, $\sim_{A-1} \sim(r \supset r) \wedge \sim_{B-1} \sim(r \supset r)$, and thus, $\sim_{A} (r \supset r) \wedge \sim_{B} (r \supset r)$? By (5) and De Morgan, either $A \neq 2$ or $B \neq 2$. In either case, the corresponding conjunct is refutable in classical logic.

So, classical propositional logic is formally inconsistent. We do indeed have this bad situation in classical logic. A pity that so much effort was poured into this system without anyone noticing the contradiction! People tried to get complicated systems of set theory that were consistent, not noticing that they had already adopted an inconsistent propositional logic at the bottom.

Remark: I have assumed a bit of classical arithmetic here, say, that if $A \neq 2$, then $A - 1 \neq 1$, and so on. Given the failure of the usual computation of $2 \cdot 2$ perhaps this is doubtful. However, I see no reason to reject these computations, and will assume them. It will be just as much a disaster if these are rejected, as the rejection of the consistency of classical propositional logic.

Also, I have used the assumption that the Putnam quantities are to be such that A (and B) = 1 or 2 (see footnote 3). Nothing except the demonstration of the inconsistency of propositional logic has really used this (even the invalidity of the usual proof that the classical theorems are all truth functional tautologies does not use this). However, in any case, the possibility that $A = 1$ or 2 (and the same for B) is certainly not excluded.

Although I write throughout the paper as if the Birkhoff and von Neumann quantum logic were a formal system in the usual sense, in fact I don't know even at the level of propositional logic whether the relation of implication (corresponding to set inclusion) between two propositional schemata using $\vee$, $\wedge$, and $\sim$, interpreted in the way suggested in complex Hilbert space (or alternatively in every inner product space over the reals or complex numbers, and so on), is or is not recursively enumerable. Nor do I know the same thing about the validity (in the sense described) of a single such schema. I do not even know whether the answers to these questions are or are not known, or have even been

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investigated, or are hard or easy. *A fortiori*, the same holds if this kind of quantum logic is extended to quantification theory.

What is the moral of this? Well, we can't just propose to revise propositional logic and assume that metatheoretic statements about formal systems are not affected. There is no bedrock in the notion of a formal system which one can choose to somehow adopt or reject at will and then tailor one's logic to it. One has to first use reasoning in order to even see what is provable in a formal system. Formal systems *don't say anything* as they are; they're just formal objects. One reasons about them using the same reasoning that one always used. If one then gives them an interpretation, one can check – using the reasoning that we always used (and what other reasoning do we have? *None*, as I'm trying to say!) – whether under this interpretation they are (*a*) sound and (*b*) complete.

What can we say about Putnam's system? If he tries to interpret his connectives as $\vee$ and $\wedge$ and $\sim$, his propositional logic can be formally set up, but will be incomplete because it lacks the distributive law which holds for these connectives. If he adds to it some extra statements like (3) – (6), his system will not only be incomplete, it will be unsound because it contains some premises that must be false. If one tries to say, 'No, no, no – you're presupposing logic: you can't *use* it in thinking about this', try and do without it and think about these things: you will not be able to do so. You will not even know what formal systems you are comparing.[42]

I haven't considered all alternatives but why not say this: there aren't different logics; there is only logic, that is, only reasoning. There are different formal systems; we use logic to reason about them. If a new formal system has some interesting informal interpretation, then it may embody sound principles of logic. But we can't *adopt* it. We can only try to prove that a given formal system embodies either (*a*) sound principles or – perhaps more ambitiously – (*b*) complete principles for some domain of connectives. There may be new connectives that we haven't thought about before and new laws which apply to them, and this may be called 'introducing a new logic', but only in a loose sense. It is really that we have found out what holds *in logic*. We may also even on a rare day – but it can happen – discover that something we thought of for centuries as a sound principle

[42] A referee objects that quantum logic is weaker than (properly contained in) classical logic, and so my arguments cannot be arguments against quantum logic *per se*. As I have argued above, it isn't really clear what formalism follows from Putnam's proposal ('reading the logic off the Hilbert space'). But the main point for Putnam and for me is that it allows one to assume things to be true that would be inconsistent in classical logic and in intuitive reasoning. So even assuming that the logic is really given by omitting the distributive law, it is not just dropping something but allowing one to assert things that could not be asserted before. Moreover, see my remarks above regarding intuitive reasoning and formal systems.

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of logic was actually based on a fallacy. This is not because we are 'adopting a new logic', but because we look at the old formal system and see that it wasn't really sound with respect to its informal interpretation and that the proof we thought we had was fallacious. This can happen just as anything else can happen. Just as our other reasoning can sometimes embody fallacies, so our proof that a given formal system is sound with respect to any given interpretation can contain a fallacy, and occasionally perhaps it has.

Also, there can be special situations in which the traditional quantifiers and connectives may need a modified semantics. Claims to this effect should be evaluated by intuitive reasoning. Thus, I regard none of various ideas as being condemned by the arguments of the present paper. For example, the idea that language with vague predicates should be handled by the Łukasiewicz unit interval semantics, or by a supervaluation idea, or by other proposals. Or truth-value gaps in a language with partially defined predicates might be handled by the strong Kleene logic, or by supervaluations, or whatever. All these ideas can be evaluated by their intuitive merits. In some cases (say the gaps case just mentioned) these need not be competing ideas, but alternative approaches.

So all of these things can happen. But if instead the game was supposed to be: 'Logic is going to be up for grabs; we will choose which logic we are going to adopt' – maybe this can mean something. I don't think sense has been given to it yet, and in spite of the common use of this kind of phrase in the literature, I don't know what it means, and I would await the further literature to see what it might mean.[43]

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